

SCIENCE

FRIDAY, SEPTEMBER 2, 1887.

THE ADMIRABLE APPOINTMENT by the President of Mr. G. Brown Goode as Commissioner of Fish and Fisheries was announced this week. It meets at once the requirements of an exacting office and the exceptional provisions of the law creating it. Professor Goode was intimately acquainted with the methods of Commissioner Baird, whose scientific zeal and knowledge he shared, and his experience and attainments in practical fish culture and in the science of ichthyology make him easily first among those whose qualifications the President has been called upon to consider. But the fact that the President has been able to select from among the civil officers of the Government a known scientist, acquainted with the habits of food fishes, to serve in this important office without extra compensation, does not remove the absurdity of this special law. If Mr. Goode should die to-morrow there is absolutely no civil officer of the United States qualified, under the terms of the act, to take his place. The special law was passed when fish culture was in its infancy. Congress was willing to risk the experiment, provided it was intrusted to Professor Baird, and framed the law accordingly. The present provisions of the law have been outgrown. It is an absurdity to have a Fish Commissioner receiving not as Commissioner but as Assistant Secretary of the Smithsonian a salary of \$300 a month, and appointing and controlling a Deputy Fish Commissioner at a salary of \$416 a month. Nor is there any reason why the Fish Commissioner should not be paid a salary commensurate with the importance of his office, and be exempted from discharging the duties of two offices with the pay of one. These matters can be appropriately considered when the Senate is called upon to confirm the new Fish Commissioner.

IN WRITING of examinations a few weeks since, we mentioned the fact that we proposed to return to the same subject later. It gives us pleasure this week to present to our readers the views of Gen. Thomas J. Morgan of the Rhode Island State Normal School, Prof. W. H. Payne of the University of Michigan, Supt. Thomas M. Balliet of Reading, Penn., and Dr. B. A. Hinsdale of Cleveland, on the function and conduct of examinations. Too many of those who are engaged in the profession of teaching look upon the periodically recurring examination as supernaturally ordained, and therefore not to be altered or questioned. For such persons, and for their pupils, an examination is a dreary routine to be dreaded. It is to be looked forward to for months, and 'crammed' for with assiduity and perseverance. It is this aspect of examinations which is specially to be criticised and combated. The proper place and scope of examinations in any educational system must be determined and understood. They must work in harmony with enlightened instruction, and not project a foreign and inharmonious element into it. We trust that the present symposium will exercise a good influence toward this end.

THE QUESTION OF A SYSTEM of improved public roads is one so closely related to every material interest of the State as to place it properly among the most important questions of public economy. The science of road making and maintaining, though neither difficult nor abstruse, is nevertheless based on principles so well established and so unvarying in their operation, as to render their thorough comprehension an essential to success in securing and maintaining public roads, at once efficient and economical, whatever the

administrative system under which they are constructed. In other countries the superintendence of public highways is recognized as an important and responsible duty, and is usually assigned to specially-trained, expert government engineers, while in the United States, where the greater mileage makes the economy, if not the efficiency, of roads even more important than abroad, the States depend for this responsible service on private citizens locally and temporarily appointed to the duty, without having provided for them the technical instruction and training so essential to success under any system. In view of this state of affairs we take pleasure in recording a move on the part of the Engineering Department of Vanderbilt University, which, under due restrictions, provides for the proper instruction, free of charge, of those who may wish to know enough engineering to make them the better road-builders.

ORIGINAL RESEARCH IN THE AMERICAN COLLEGE.

OUR American colleges, with the exception of a few of the larger institutions, are unfortunately not places of original research. It has hardly seemed to have entered into the American idea of education that a college, besides being a place of instruction, should be the place for the origin of new knowledge. Of late years, however, the influence of German universities, and of some of the larger colleges in this country, has been creating the conviction that original research in some form is necessary for the life of our higher educational institutions. There are thus numerous indications that the future is to see our colleges more the home of new learning than they have been in the past. But while we are beginning to realize how greatly it is for the interest of our colleges that research should be carried on within their walls, the prospects are, that, until a complete change takes place in our system, such research will be confined to the instructors and graduates, and will not be shared in by the undergraduate student. With a few exceptional cases we find the attention of the undergraduate confined to routine work, and it is only after graduation that he is allowed to specialize so far as to take up original investigation. Now, while this is due partly to lack of facilities and opportunities, partly to lack of requisite knowledge on the part of the instructor to direct such work, partly to the difficulty of selecting work which a young student can do, and partly to the universal disinclination to make new moves, it is at the same time largely due to a more worthy reason than any of these. There are many instructors in our colleges, who have every facility for such work, who think it not wise to encourage it, even though it would make the personal work of teaching a much more congenial one. It lies outside the scope of our college course. While, then, we may hope to see a time in the not distant future when our colleges shall be places of research, it is very doubtful whether this research will ever be shared in by the undergraduate, except in isolated cases.

The reason for this lies in the peculiarly American idea of the scope of and necessity for what we call a liberal education, and not in any failure to recognize the value of research. The value of research as a means of education in stimulating the student is fully appreciated. It tends to counteract many evil tendencies of our college-work. Routine courses in science as ordinarily pursued are apt to become monotonous and tedious to the student, soon degenerating into mechanical work. With the experiments detailed for him in his text-book or laboratory directions, their results cease to interest him, and a careless habit is almost sure to be fostered. His thought is hardly stimulated at all, but is rather curbed by the feeling that he is going over a path which hundreds have followed before, and that consequently his discovering any thing new is an impossibility. It is indeed surprising to see what little thought is required, on the part of the student, to go through some of our routine science courses. He learns the text-book, mechanically

performs the experiments, and notes results, but the amount of personal thought which is stimulated is sometimes infinitesimal. Now, since the design of our college course is to make the thinking man, it is plain that something is needed to offset these tendencies, and some of our educators would find this something in original research. That a bit of research will counteract these evils in large measure is certain enough. When the student is engaged in solving some new problem, the whole aspect of his study is changed. It is no longer tedious and dull, but interesting and full of life. A new world of thought is opened, and scientific methods of thought and work become engendered. Travelling a new road, the student is ever on the watch for new facts and thoughts. Carelessness and mechanical labor are no longer possible, for he soon discovers that his success depends upon the amount of care and thought that he puts into his work. In short, original research soon does away with the school-boy, and makes the student in its best sense.

But while the value of this method of study is apparent, it by no means follows that such a course is best for our college student. Most American instructors believe at present that the evils resulting from such a course would more than counterbalance its advantages: for it is plain enough that a course founded upon research will tend to make specialists, and nothing else; and this is not the design of our colleges. The American college is quite naturally compared with the German universities; but the comparison is an utterly false one, for the aim of the two is entirely different. The German university student is studying a special course, for a special purpose, and is usually looking forward to government employment. The government appoints ministers, doctors, lawyers, teachers; and eighty per cent of the university graduates obtain such employment after passing a rigid State examination. It is this examination and government employment which serve as the inspiration of the student. The German Government has decided that a high education shall be the means of entrance into the upper circles of society. For a German who is not born noble, there is only one way of gaining an association with the upper classes, and that is by a university education and government employment. With such an inspiration, Germany could hardly help developing a high grade of education and early specialization. Considered in the light of education alone, her system has certainly been the most successful ever instituted. But in this country the conditions are very different, and such an educational system is both impossible and undesirable. Our government has set no premium on education, nor are our professional men at all dependent upon the government for success. They are dependent directly upon the people, and therefore upon a hundred unforeseen possibilities. There are other ways of improving one's condition and rising in the estimation of society than by becoming professional men, for many other paths of life are equally respected. We have no educated aristocracy, for we believe there are other good things besides learning. Our colleges have therefore a broad function to perform in meeting these conditions. They desire, not to make specialists, but to train men. They do not desire to make ministers or doctors or lawyers. This is left to professional schools, which therefore compare more nearly with the German university. The college course is one which we think a student should have before beginning a special training, believing that such preparation will result in making better professional men, better business men, and, above all, better citizens. It is the general training of lower schools broadened and expanded. German university education tends to unfit men for any course of life outside their specialties, and Germany is becoming alarmed over the increasing class of educated men who cannot obtain government employment, and are fit for nothing else. The same is true of our professional schools and advanced courses in higher universities; for they, too, fit men for narrow courses in life. But the college tries to put men in a position where they are better fitted for any path in life, from farmer to statesman. The professional school tries to make the scholar; the college, to make the man. That our plan of education is not adapted to the production of the largest number of gifted scholars may be very true; but that it is best adapted to the needs and demands of our system of society and government is the firm belief of most American educators.

Now, if we recognize this as a worthy aim of our colleges, we shall understand why original research is not encouraged on

the part of undergraduates. We hear a constant demand in this country for a liberal education. It is for this that our students go to college, for this that the course is planned. It is planned to occupy four full years, with little time left for outside work. Fortunately, however, this idea has not been strong enough to oppose successfully the introduction of the elective system, for this freedom of choice has fought its way into all colleges. Our education has been vastly improved by allowing the student the privilege of devoting his energies toward a line of studies congenial to him. This system of greater freedom is yearly widening its scope, and it is of course impossible to tell where it will end; but there is no reason for thinking it likely to go beyond a broad freedom in electives. To take another step, and introduce the earlier and narrower specialization which would result from the encouragement of original research, would be to abandon completely our belief in the value of a liberal education; for the investigator becomes the specialist from the start. His attention is withdrawn from other subjects, and, with the American's hurry to do something, he is almost sure to neglect completely all lines of learning except his own. We do not want our colleges to develop classes of men who are good for nothing outside of one line. For the general student, then, narrow specialization is always injurious. Still further than this do our colleges go, insisting that even more should the student who aims at a special line of work be cautioned against taking it up too quickly. The only time that he will ever get for acquiring knowledge of other departments of knowledge is during his college course; for, as soon as he takes up his own, he will pursue it with an eagerness which will hide all else. To begin to specialize early seems at first sight a gain in time; but it is in reality an irreparable loss, for it is beginning to build without laying a sufficient foundation. To make the best sort of a scholar requires more than an exhaustive knowledge of one thing: it requires a broad knowledge of thought. It is quite common, therefore, to find our professors recommending their own students to keep outside of their specialties as long as possible. Practically, too, is it important, for our educational positions demand it. Our institutions are not yet highly enough differentiated to offer work for very narrow specialists. We have many colleges and broad departments. They want teachers in biology, and not in entomology; teachers of physics, and not of electricity. In short, the demand in the country to-day is almost everywhere for broadly educated men, and not for the narrow students which are the inevitable result of early attention to original research. The general training the colleges must give, all special training being reserved for the professional schools and the universities. If our colleges fill this demand, they will continue to exert a powerful influence; but, if they cease to do so, the American college will disappear, and its place will be supplied by professional schools and universities. To convert our colleges into universities is impossible, since our students have had no thorough training to start with, which corresponds to the German gymnasium. In attempting to avoid the old plan of rigid courses, and by electives introduce more freedom for the student, our colleges have vastly improved our system of education. Whether or not this idea has been carried to excess is still a matter of dispute, as can be seen by comparing the views of the representatives of our two leading colleges, Harvard and Yale. But the opinion would be almost unanimous that a system which enables the student to so devote himself to one subject that all others are lost sight of, belongs not to the American college: it belongs to the university or the professional school. At all events, our colleges at present are planned to give the student a liberal education, and not a special training; and for this purpose routine courses, and not original investigations, are adopted.

Even with our elective systems in vogue, there are, as we have seen, two equally undesirable extremes. On the one hand, routine courses tend to degenerate into mechanical work and monotony, curb original thought, and generate carelessness. This is the most common fault of our American colleges. On the other hand, original research on the part of the young student encourages too early specialization, and thus defeats the plan of a liberal education. This is not as common in this country. But neither extreme is necessary; for a medium course is possible, which shall in a measure avoid the evils of both extremes, and give the advantages of both.

systems. Such a course we must regard as the most promising one for the future.

Doubtless we shall always meet with undergraduate students who are engaged in original research, perhaps in the future more frequently than at present; for there are certainly instances where such a method of instruction is best. It may be in the case of a student whose habits of carelessness require some measure for correction, or whose utter dependence upon teacher or text-book requires vigorous offset. It may be the student whose general ability is manifest, but whose interest in study has never been awakened: to him a problem of research may prove a lifelong blessing in rousing his slumbering energies. It may be the student who has determined to pursue some special line of scientific work, requiring no general learning, as that of an analytical chemist, or that of some of the government scientific staff: about all that can be said in this case is, the quicker one begins his work, the better. It may be the student whose general faithfulness elsewhere makes it evident that he may take up special work in his own chosen line without detriment to his general education; or it may be in other cases, where there seems to be a particular reason for it. But at most these instances will be few in number, and such work cannot apply to the bulk of students who take science courses in our colleges. For this larger class some sort of routine class-work is necessary; and the question arises whether it is not possible to so arrange it as to avoid the evils which have been and still are too largely attendant thereto. That this is possible is proved by the fact that it is already done in many places.

The methods of conducting the ordinary routine science courses in our colleges at the present time are extremely varied, ranging all the way from simple text-book recitations to plans which involve a large amount of independent work on the part of each student. Experience seems to warrant the statement that the nearer such work approaches the nature of original investigation for each student, the more successful it is in arousing his interest and stimulating his thought. But how is it possible to combine both plans? In many places in this country we find the adoption of plans adapted for this purpose, and implying the minimum amount of what we have called routine work. In the first place, text-books are cast aside, except as books of reference or as serving to give details. The student is thus made to look directly to his instructor for information. This system of lecturing or talking has been borrowed from Germany, where it is the only method of instruction, and is capable of yielding the most excellent or the most evil results, according to the faculty of the lecturer. Where it is simply repeating verbally to the class the substance of some text-book, as is sometimes done, it is much worse than giving the text-book to the student to learn. But where the lecturer's wide knowledge of his subject enables him to collect material from numerous sources, and he is able skilfully to arrange it in such a way as to lead the student from one principle to another, it will give more true knowledge than any text-book. The chief reason is its flexibility; but an equally important consideration is that it enables the lecturer to introduce inferences and conclusions, in the midst of the course, where they belong as drawn from the facts. He can pass from fact to inference, from inference to theory, at will; and he is not obliged to crowd all the principles of his science into an introduction to the course, where they will not be understood, or at the end of the course, when they have lost their interest, one of which unfortunate plans is considered necessary in all text-books. To be sure, we find good students not uncommonly objecting to lectures. This is largely because they are anxious for something to learn; for such work they have been taught to do. They find it difficult, however, to comprehend the meaning of a course which uses details only for their significance, and aims at principles of science rather than detailed information. They know how to learn a lesson, for this they have been taught; but they do not know how to think. In a large class in physiology, scarcely one failed to give the names of the microscopic layers of the retina, though not asked for them and distinctly told that they were of little importance to remember. But quite a number utterly failed to comprehend the significance of the eye as an optical instrument. The former was something to learn; the latter, something to think out. Of course, all students cannot be made to think; but, while it is impossible always to cor-

rect this error of learning simply by rote, it is certain that the lecture system, when wisely conducted, tends to correct it, while text-books tend to foster it. It is very seldom that an instructor who has once tried the lecture system gives it up, except for matters of detail. It helps to avoid old ruts, and insures the instructor that for a time, at least, the students are thinking vigorously, a fact of which he can never be sure with the use of text-books. It is one of the important means of bringing the student in contact with science itself rather than with second-hand learning.

Our science courses are now almost universally accompanied by a certain amount of practical laboratory-work, and here it is possible to enforce a great amount of independent observation and thought. Text-books, except in the form of a library of reference, can, if desired, be completely eliminated. A series of experiments following a course laid down in a text-book is of about the same value as experiments performed before the class, and not very much more; for, with the details and results given him, the student usually is concerned only in performing the experiment successfully, and not in thinking of its significance. But under the direct supervision of a wise instructor, each person's laboratory-work can be so planned as to force him to draw conclusions, and make his own discoveries. He may be told little or much, as the case may require, but always just enough to set him on the track of seeing more for himself. His experiments may be modified to suit emergencies. Numerous minor problems may be set for his solution, for which he has no answer, nor can find one except by studying the experiments, or as his instructor may be inclined to assist him.

An illustration or two may serve to make this more intelligible. A student studying biology is given a fish's skull to examine, with the aid of the instructor's directions, or some book of reference. After thoroughly mastering this, he is given in succession the skull of a frog, an alligator, a turtle, a bird, and a series of mammals' skulls, beginning with the opossum and ending with man. These he compares, in turn, with the fish's skull and with each other, using no books, but simply studying the skulls, with here and there a hint from the instructor, as it may seem desirable. Such a comparison can be completed with sufficient accuracy in a few days, and proves invariably of interest to the student. By the time it is completed, not only has he gained a good idea of the vertebrate skull in such a way as to remember it, but he has discovered for himself the spirit of comparative morphology in such a way that it can never escape him. Or, again, he may be given a book or a lecture describing a lobster, and given for dissection, not a lobster, but a crab; or he may be given a few simple animals, and asked to arrange them in what he would regard as natural groups. These illustrations I have taken from the department with which I am most familiar, but there is no difficulty in applying the same plan anywhere. There is an endless variety of such work, which shall compel the student to combine thought with the mechanical work of the laboratory. Nor does this plan prove as slow as it would at first sight appear. Perhaps it is slow at first, but the rapidity increases with every advance. Work can be constantly varied from one subject to another so as to conduct the student over the whole ground desired. It can be varied with the individual; and, if there be occasional lectures accompanying the practical work, there is no difficulty in covering all the ground necessary.

Such a course is simply the 'object method' adapted to a larger scale, and it has many advantages. It trains the observation, develops carefulness, stimulates original thought. It gives the student the elements of science, but in such a way that they mean more to him than when obtained by any easier plan. It gives him the satisfaction of feeling that he is accomplishing something on his own part, but does not so distract his attention as to injure his work elsewhere. It fits in with the requirements of a general course, and at the same time offers some of the advantages of the German system of independence on the part of the student. It is the best sort of science for the general student, for it gives him not only facts and principles, but some understanding of scientific methods of thought and observation, which is more valuable than the facts. It is the best method for the student who is to make the science his specialty; for, while introducing him to the elements, it familiarizes him with the spirit of scientific investigation, and shows him that nature is the text-book from which he is to learn.

It is plain enough that there are serious practical difficulties in the way of such courses. The chief one is in the amount of time required from the instructor for their successful management. It not only requires that the instructor should have a complete acquaintance with his subject, but requires a constant personal supervision and thought, constant variations with different students, and requires that each instructor should plan his own course. A textbook is impossible, for it defeats its own end; or, if one instructor should write a book for his own class, it would be useless for others. Indeed, it is hardly possible to have any definite course; the aim being that each student should be brought in contact with the principles of nature as best suits his own ability, and not that the class as a whole should go over a regular course. Such work is by far the most difficult sort of teaching; and with the present small faculties of many of our colleges, and the inadequate training of many of the professors, it is practically impossible. But happily the faculties are growing larger, and more and more attention is being paid to selecting instructors fitted for their departments by previous training. Fewer hours of recitation-work are demanded, and more time is left to our instructors for thought and personal teaching. In many places can be seen a constant growth of this personal contact of instructor and student, and as fast as it grows we see the routine work of classes replaced by the work of students as individuals.

Along this line, then, we may look for the future development of sciences in the American college. We may hope for an increase in the amount of original investigation; but this must come chiefly from the instructors and graduate students, and it will then serve as an inspiration to the college. We may look for larger laboratories, more apparatus, and greater facilities for practical work on the part of large classes of students; but this will be insufficient unless we see at the same time an increase in the corps of instructors. Our boards of instructors should be large enough to make possible some personal supervision of the students, so that the individual will not become swallowed up in the mass, and large enough to allow to the instructors some time for research, by which means alone they can keep apace with the times. The great demand of higher education in this country is, therefore, not for more colleges or more buildings, but for more money devoted to instruction.

H. W. CONN.

THE FUNCTION AND CONDUCT OF EXAMINATIONS.

THE professors of a German university do not assign the student lessons, or require him to hear lectures. When the time comes to grant or refuse him the degree, their sole sources of information as to his fitness to receive it are, the thesis that he hands in, and the examination to which he is subjected. As respects time, this is a system of unlimited election. That it develops splendid qualities in the student; that it is very grateful to young men who love freedom and hate task-work; and that, together with the other features of the German system, it produces scholars eminent in every branch of scholarship, — are well-known facts. In a German university, stated work is at a minimum, and the examination at a maximum, as a test of proficiency.

At the opposite end of the scale are the primary schools, in the strictest sense of that term. Here no election of work or time can be allowed beyond what extra-school conditions call for. 'Cutting' is absolutely inadmissible. The teacher cannot wait until the end of the term or month, or even day, to discover what the pupil knows: he must prescribe work every day, and, at the beginning, every hour, and then see that the work is done. This is a maximum of lesson, and a minimum of examination.

So far, all is plain and easy. But the moment that we enter the grades of school-work lying between these extremes, we meet a wide difference of opinion, and encounter serious practical difficulties. Here Germany has nothing to teach us. The method of the primary school is then continued to the end of the gymnasium course, when the student plunges at once into the fullest university liberty. The proper end is, rather, progressively to lift the pupil above the task-work level, to give him freedom, and to make him self-reliant. Two opposite tendencies are now very observable in the United States: —

1. A considerable number of colleges are allowing a limited election of time. This means, if a proper regimen is maintained, less dependence upon the daily recitation, and more dependence upon the examination.

2. In the intermediate public-school grades there is a diminishing dependence upon the examination, and an increasing dependence upon the daily work, particularly when the time comes to make the promotions: in fact, this tendency is declaring itself all along the public-school line.

These tendencies are both good; something of the freedom and enthusiasm of the university is finding its way into the college; and there is a manifest slackening of the high public-school tension of a few years ago, that was brought about by the abuse of examinations. Good results may be expected from both these movements.

The adjustment of requirement and election, of stated lessons and examinations, above the primary grades and below the college, or possibly the university, is a problem that every teacher and superintendent will be called upon to solve anew. The elements will vary, and no formula can be given. The solution in a given case will depend upon the facts that condition the home, the school, and even the individual pupil. It is often urged against examinations that they promote cramming. Teachers who have to solve this problem will do well to remember that they also tend to prevent cramming. Pupils cram for the daily recitation as well as for the examination; and as the daily recitation tends to check cramming for the one purpose, so the examination tends to check it for the other purpose.

B. A. HINSDALE.

I AM asked to write a very brief article on the function and conduct of examinations. By examinations is meant a formal set of questions answered in writing. Among the useful purposes which can be subserved by such tests are the following: —

1. They may serve as a stimulus or incentive to study. Students who know that at some period of their work they will be required to give written answers to questions based on the work done are likely to be more attentive, industrious, and interested in their work.

2. They encourage thoroughness. Those who prepare for an oral recitation may depend upon chance, or artifice, or favoritism, to help them through; but a searching examination, calling for exact written statements, is another matter, and demands better preparation.

3. They afford an opportunity, in some instances, for a review of the whole subject passed over during the term.

4. They are often valuable as an exercise in English composition, calling as they do for clear, concise, comprehensive statements.

5. They are a revelation to the pupils of their own ability and attainments, as well as of their weakness and defects.

6. They call for concentration of mind, sustained mental effort, and a ready use of one's resources, which is a valuable educational discipline.

7. They reveal to the teacher the results of his teaching, the failure or success of his methods, and thus afford an opportunity of modifying his work when necessary.

8. The tabulated results of a series of examinations, extending through several months or years, indicate with considerable certainty the student's trend of mind, habits of study, and scholarly development. These results are specially valuable to parents in deciding what is best for their children.

9. The results are helpful to superintendents and others in forming an opinion of the progress of the pupils, and the work of the teacher.

10. They give to school-work a kind of dignity, increase the student's self-respect, and impart to the teacher's mind a judicial habit, freeing him from the great tendency to judge of his pupils by sentimental regard rather than by a critical judgment.

With these ends in view, how shall the examinations be conducted?

1. They should be an ordinary, and not an extraordinary, part of school machinery. If they are held only at the close of a term, or at the conclusion of a study, the students should be prepared for them by the character of the daily recitation, and by occasional

written recitations, and 'tests,' which resemble the written examinations, but are less severe.

2. The purpose and method of the examination should be fully explained to the pupils, and their mistakes and failures should be pointed out.

3. The questions set should be adapted to the age and ability of the pupils, easy enough to encourage them to attempt all, and difficult enough to call for their best efforts; should pertain to the work actually done; should be explicit, concise, logical, and call for thought and a mastery of principles, as well as for memory.

4. Too much importance should not be attached to the results. They should be reckoned as only one element, among several, in determining the standing of the student, and his fitness for promotion or graduation. They should never be made the basis of ranking, or the sole ground of promotion.

5. They should always be regarded and treated as simply one means or device in the process of education, and should never be treated as if they were the goal to be gained. They are a means, and not an end.

6. They should not be so severe or prolonged as to overtax the students' powers, should be conducted with absolute fairness and impartiality, as well as with good sense in regard to time, place, and circumstances, and proper allowance should be made for any exceptional circumstances, such as illness on the part of the student. The 'final' examination should be held long enough before the close of the term to allow the teacher to make the proper use of the results before the class separates.

THOMAS J. MORGAN.

IN the current discussions on the use and the abuse of examinations, it seems frequently to be assumed that their one great purpose is to test,—to furnish a basis for estimating the pupil's knowledge and ability, and the teacher's skill and success in instruction. If this were really the only purpose they serve, it would be easy to justify them, notwithstanding the fact that some evils undoubtedly flow from them. These evils do not exist in examinations *per se*, but are faults of administration; and, if a teacher proposes to abolish them on the ground that they encourage immorality, he utters an indictment of his own professional skill.

But admit, with certain extremists, that examinations have no justifiable use as tests; that, for example, a pupil's fitness for promotion, or for learning a subject, is best determined by the teacher's personal knowledge, without any formal test: even then the examination can hold its ground, regarded either as a motive or as a discipline.

I feel sure that my experience in the management of public schools has taught me that the intellectual tone of a school cannot be kept at the proper pitch by any other motive. Even the best of pupils need to feel that they must study with a view to rendering a formal account of their opportunities. Here, again, the stress may be too great; but this is simply a fault of administration, which is a direct reflection on professional skill.

But leaving also the motive power and value of examinations out of account, they have a third and adequate defence in the fact that they afford a discipline of incomparable quality. The ability to render a clear, exact, and comprehensive account of what we know on a given subject, under some stress, or in view of something important depending on the result, is an endowment of supreme importance; and I know of no instrument for this purpose save a judicious examination.

As it seems to me, the only debatable question in the case is that of use and abuse: it is simply a matter of administration.

W. H. PAYNE.

THE purpose of all education must be the development of thought and character in the widest sense of these terms,—“the generation of power.”

Examinations are of use only in so far as they are in harmony with this general purpose. They are a great power for good or evil; they may be made a blessing or a curse to schools. An examination of pupils conducted by a supervising officer should have a threefold aim:—

First, It should be made a test as far as the pupils are concerned. Right here comes the danger of all examinations. What shall the

test be? Quantity of knowledge? Then ‘cramming’ will be the inevitable result; and the superintendent who thus plants thorns and thistles has no right to expect them to bring forth grapes and figs.

The examination must test power, and not mere quantity of knowledge,—power to do intellectual work. As all food that is eaten is not converted into physical force, so all knowledge acquired is not converted into mental power. The test-question in the former case is, not “How much have you devoured?” nor even “How much do you weigh?” but “How much can you lift?” So in the latter case it must be, not “How much do you remember?” but “How much can you do?”

The examination, therefore, should not require a mere reproduction of what the pupil has learned, but it should test his power of dealing with new questions and problems closely allied in principle to those which he has studied.

Secondly, It should stimulate the pupil to work in right lines. The pupil will work for the examination, and it is right that he should. If by working for it his work is wrong, the fault lies with the examiner and the examination. A superintendent has it in his power in this way to direct, in a large measure, the study of all the pupils in his schools.

Thirdly, It should test the character of the teaching, and should direct the work of the teacher. The teacher, like the pupil, will work for the examination to a very large extent. That which is made the chief test in the examination will be the motive of work with both teacher and pupil.

The examination is therefore a powerful lever, in the hands of a competent superintendent, to force school-work into right lines.

THOMAS M. BALLIET.

THE ASSOCIATION OF OFFICIAL AGRICULTURAL CHEMISTS.

THE fourth annual meeting of this Association was held in the Library of the Department of Agriculture, Washington, beginning Aug. 16, and lasting three days. The president, Dr. E. H. Jenkins of Connecticut, in his opening address, congratulated the Association on the success which had attended its efforts in securing the adoption of uniform methods for the analysis of fertilizers. He also recommended that the Constitution of the Association be amended so as to include chemists of agricultural colleges, and all official chemists having control of fertilizers, dairy products, and agricultural products in general.

The first business of the session, after listening to the President's address, was the reception of the report of the committee on fodders and feeding-stuffs, of which Prof. G. C. Caldwell of Cornell was chairman. The committee had sent out a number of samples of fodders and feeding-stuffs for comparative analysis, and the results obtained were presented. They showed that in the same sample widely different results were obtained by different analysts. These variations showed the necessity of adopting a strictly uniform method of analysis. Such a scheme was reported by the committee, and, after discussion and amendment by the Association, was adopted to be used by all analysts connected with the Association during the coming year.

The report of the committee on dairy products was presented by Dr. H. W. Wiley of Washington. The committee had sent out various samples of butters and butter-substitutes for examination by members of the Association. The tabulation of the analyses, as in the preceding cases, showed wide variations in many particulars. After discussion and amendment, the following method of analysis for butter and milk was adopted. For butter, preliminary examination with polarized light and selenite plate was recommended, while it was stated that the melting of butters and butter-substitutes, and their subsequent examination after cooling by polarized light, appeared to have no value as means of qualitatively sorting butters and butter-substitutes. The method of determining the specific gravity of the butter-fat at 40° C., in a picnometer, was adopted. For Reichert's method, the saponification is to be made in the flask to be used subsequently in the distillation, saturated solution of potash with a small amount of alcohol to be used, and the fat-acids subsequently to be freed by phosphoric instead of sulphuric acid.

For the estimation of fat in milk the Adam's method is recommended, viz., absorption of the milk by bibulous paper, drying, and extraction with ether. As alternate methods the procedure of Morse and Piggot or the lactocrite may be used. In the method of Morse the milk is dried by treatment with anhydrous sulphate of copper, and the fat extracted with light petroleum. Afterwards it is estimated volumetrically by saponification with standard alkali. In the method by the lactocrite the fat is separated in a centrifugal machine, revolving at the rate of 7,000 turns per minute, the milk being previously treated with an equal volume of a mixture of 1 part sulphuric and 20 parts acetic acid.

The method for analysis of fertilizers, with a few slight changes, remains as last year. The most important contribution in this matter was from Prof. M. A. Scovel of Kentucky, who showed that fertilizers containing nitrates could be treated by the Kjeldahl process, if the sulphuric acid used in digestion contained a certain portion of salicylic acid. By this means the total nitrogen existing in the three forms can be determined by an extremely simple and easy process.

The meeting was largely attended and full of practical interest from beginning to end. Two new committees, viz., one on fermented liquors and the other on sugar analysis, were appointed.

The following are the officers and committees for the coming year: President, Dr. P. E. Chazal; Vice-President, Dr. W. J. Gascoyne; Secretary, Mr. Clifford Richardson; Members of Executive Committee, Dr. E. H. Jenkins, Prof. J. A. Myers; Committees, on phosphoric acid, W. J. Gascoyne, N. W. Lord, W. E. Moses; on nitrogen, M. A. Scovel, N. T. Lupton, Wm. McMurtrie; on potash, J. A. Myers, Wm. Frear, E. H. Jenkins; on feeding-stuffs, G. C. Caldwell, W. H. Jordan, Clifford Richardson; on dairy products, H. W. Wiley, S. M. Babcock, H. P. Armsby; on fermented liquors, W. B. Rising, C. A. Crampton, G. F. Fellows; on sugar analysis, W. C. Stubbs, N. T. Lupton, H. W. Wiley.

ALASKA LETTER.

It is strange that so little is known in the United States about Alaska. It has been a possession of our government for twenty years, and even now interest in it is only beginning to be developed. Yet in it we have by far the most remarkable of all our territories. Its area is not less than 600,000 square miles, or one-fifth of that of the United States proper. It is equal in extent to all the New England states, all the middle states, Ohio, Indiana, Michigan, the Virginias, the Carolinas, Tennessee, Kentucky, and Mississippi. Sitka is as far from the parallel of the extreme western boundary of Alaska as it is from the parallel of Eastport, Me. The present governor of the territory estimates its population to be 35,261, including whites, creoles, and natives. Of this number, 10,600, including 3,100 whites, dwell in south-eastern Alaska, the part accessible to tourists.

The native race of south-eastern Alaska is the Thlinket. The Thlinkets are far superior, intellectually and industrially, to the North American Indian. They are variously said to be of Asiatic and Aztec origin, but the majority of observers believe them to be related to the Chinese. They are skilful workers in wood and metals, shrewd traders, and very amenable to civilizing influences.

The climate of south-eastern Alaska is any thing but Arctic. The observations of Sergeant John J. McLean of the Signal Service at Sitka, for the year ending Aug. 31, 1886, showed an average temperature of 44°.8 F. The maximum was 72°, reached both in July and August, and the minimum 4°, reached in January. The rainfall is very heavy, often being more than 100 inches per annum.

Alaska's resources are timber, mining, furs, and fisheries, but as they are only just being measured, it is useless to quote figures concerning them.

The trip to Wrangell, Juneau, Sitka, and the great glaciers of south-eastern Alaska is now easily and quickly made by frequent steamers from the Puget Sound ports. During the summer season there are weekly sailings, and the fastest steamer makes the round trip from Tacoma, W.T., in eleven days. Travellers should provide themselves with warm clothing, for it will be needed during the entire trip. Rubber boots or overshoes, a rubber coat, and a stout pair of walking-boots are desirable. The last-mentioned are

necessary for climbing on the Davidson and Muir glaciers, and the rubber articles are a protection against the wet weather.

For maps of the coast, the British Coast Survey maps are to be recommended, and the 'Coast Pilot' is a most valuable aid in determining the various peaks, glaciers, and channels. Though numerous books on Alaska have been issued, no one of them is satisfactory. Lieutenant Schwatka's book does not treat of south-eastern Alaska, and those that profess to do so are superficial and inexact. Hubert Howe Bancroft's 'History of Alaska' is important, but far from satisfactory.

Adequate educational provision for the native and white children has yet to be made. The Thlinkets show great ability in industrial work, and it is a source of great satisfaction to hear that an organized course of industrial training is to be put in operation in the Sitka school at once. In the 'Circular of Information' of the Bureau of Education known as No. 2, 1882 will be found an interesting paper on 'The Neglect of Education in Alaska.' B. N.

Sitka, Alaska, Aug. 1.

HEALTH MATTERS.

AGAINST BERGEON'S TREATMENT. — Dr. Townsend and Dr. Hennessy report, in the *Albany Medical Annals*, nine cases of phthisis treated by gaseous enemata, after Bergeon's method. The reporters say that these cases, though few in number and somewhat incomplete, are deemed worthy of publication, as showing that this method of treatment seems as much of a failure in this dreaded malady as are others equally highly advocated at the present day. Besides these cases, four others have come under the observation of the writers, in three of which they personally superintended the administration of the gaseous enemata, the fourth being seen only once in consultation, but the records of which were accurately kept by the attending physician. With reference to all these cases, it is stated that after a fair trial of from two to four weeks it was deemed expedient and proper that it be discontinued for the two following reasons: first, it did no permanent — indeed it might almost safely be said not even transient — good; while, second, it was most disagreeable and annoying to the patients, who generally were the first to suggest, or even beg for, its withdrawal.

CHLOROFORMING WHILE ASLEEP. — In the August number of the *New Orleans Medical and Surgical Journal* is an editorial comment on the subject of chloroforming persons while asleep. The editor says that there are several points relating to the physiological action of chloroform which have an important bearing on the question. The condition of health and the age of the person are matters to be considered in regard to the possibility of chloroforming people while asleep. To adults in perfect health chloroform is a decided cerebral stimulant, and it may be stated as a rule, to which the exceptions are exceedingly rare, that healthy adults cannot be chloroformed while asleep, unless their sleep has been induced by exhaustion or hypnotic agents. Weakly adults and children take chloroform with less resistance, as the stimulant effect on the cerebrum is less in degree and shorter in duration. Weakly adults and those acutely exhausted by disease or injury may be chloroformed during sleep. Children may also be chloroformed while asleep, and especially if they are depressed on any account. The editor recently demonstrated to several physicians the ease with which chloroform could be administered to a sleeping child when in a state of depression. The case was one of cancer of the mesentery, in which the little patient had been exhausted by pain and restlessness. The victims of chloroform at the hands of burglars are usually at the time in good health. The more improbable, then, is the story usually told of such burglaries. Under all conditions anæsthesia by chloroform can be accomplished during sleep only by skilful administration. Overdosage at the outset will certainly awaken the sleeper. The ability of burglars to force the anæsthesia of several persons sleeping in the same room without raising an alarm is to be doubted. In regard to the impression which prevails that burglars impregnate the air of an apartment with chloroform vapor, so as to gradually anæsthetize all the sleepers at the same time, the editor says that the weight of chloroform vapor and the readiness with which it descends make it difficult to saturate the air of a sleeping apartment, especially one

at the time well ventilated. Besides, the quantity of chloroform necessary to saturate the air sufficiently to produce anæsthesia is very considerable. Allowing one and a half grain of chloroform to the cubic inch of air, it would require thirty-eight fluid ounces to sufficiently impregnate the air of a room ten by twelve feet, with a ceiling eight feet high. It would certainly take a considerable time to vaporize this quantity of chloroform, to say nothing of the probability of awakening sleepers by any act of atomization, and even if it should succeed, what would be the effect on the burglars themselves?

MENTAL SCIENCE.

Why do we Sleep?

IN an address to the Anthropological Society of Brussels, Prof. Leo Errera has given a *résumé* of some points in the chemical theory of sleep. The phenomena of sleep have in common with other vital functions the character of periodicity. An examination of such periodic functions in general may aid in ascertaining the cause of sleep. The respiratory rhythm is regulated by the amount of oxygen and carbonic acid in the arterial blood. When the blood is charged with oxygen the respiratory centre momentarily suspends activity; but soon the tissues yield their oxygen to the blood, have it replaced by carbonic acid, and the blood thus modified acts as an excitant to the respiratory centre. Ranke has shown that the fatigue and recovery of muscles is due to a similar alternation of the accumulation and discharge of certain 'fatiguing substances,' chief amongst which is lactic acid. An injection of this acid into fresh muscle renders it incapable of work; washing the acid out restores the activity. Cannot sleep be explained by a similar chemical theory? Preyer has extended the views of Binz, Obersteiner, and others (who all agree in making the accumulation of certain products of fatigue — *ermüdungsstoffe* — the cause of sleep), by calling all such fatiguing products of activity 'ponogens.' These accumulate in waking life, are readily oxidizable, and absorb the oxygen intended for glands, muscles, and nerve-centres, until action is impossible and sleep sets in. Gradually the ponogens are destroyed by oxidation, slight excitation is sufficient to arouse the centres, and waking life begins. Amongst the ponogens, Preyer counts lactic acid as the chief, but the experimental demonstration of this has been unsuccessful, and the theory, accordingly, not generally adopted.

Since these researches Armand Gautier has found in the human body a series of five organic bases akin to creatine, creatinine, and xanthine, and calls them 'leucomaines' and 'ptomaines.' The physiological properties of these substances are narcotic, fatiguing, and sometimes lead to vomiting. This is just what the chemical theory requires. The periodicity of sleep would be explained by the conservation of energy being applicable to all bodily activity: work must be followed by repair; life is a slow suicide. There is, moreover, reason to believe that the action of these leucomaines is a direct one upon the brain; it is a direct intoxication of the brain-centres.

A theory of sleep must take account of three factors, work, fatigue, and sleep. The chemical theory satisfies these demands. All work, muscular or cerebral, produces waste products. These accumulate, make work more and more difficult: this is fatigue. As the process continues, the waste-products, notably the leucomaines, intoxicate the higher nerve-centres (just as a dose of morphine does), and render them incapable of action: that is sleep. The picture is, however, much more complex. There is a constant struggle against the fatigue, which for a time, by dint of hard work shown in increased secretions and so on, may succeed. We probably never arrive at the extreme limit of work; the sensation of fatigue intervenes to prevent such a disaster. Fatigue, as is well known, may extend from muscle to nerve, and from nerve to nerve-centre. We may be very tired from repeatedly lifting a weight, and not be sleepy, and may be generally sleepy without any considerable local fatigue. One is peripheral, the other central. As the waste products accumulate in the centres, motion and sensation become more and more sluggish until the time comes when the ordinary stimulation no longer arouses them, and we sleep. Partial sleep can be similarly explained. The centres go to sleep in a hierarchical order, the highest serving the most delicate function

going first. In waking, the reverse is the case; the motor centres may be asleep while the intellectual centres are awake. In somnambulism the latter may be asleep while the former are awake.

The depth of sleep according to this theory ought to be proportional to the number of cortical molecules in combination with the leucomaines. In the beginning of sleep these are abundant, the cerebral cells inactive, and a combination easy. The sleep is deep. Soon the maximum number of combinations is reached, and sleep is deepest. From here on, the leucomaines are gradually eliminated and destroyed, and sleep should decrease with a decreasing intensity. Kohlschütter's experiments on the intensity of sleep, as tested by the noise necessary to awake the patient, gives the curve for the intensity of sleep corresponding to what we should expect by our theory. Variations in our sleep caused by an excess of work, etc., are evidently similarly explicable. In short, fatigue is a poison for which sleep is the normal antidote.

This theory maintains (1) that the activity of all the tissues (and primarily of the two most active, the nervous and muscular) gives rise to substances, more or less allied to alkaloids, the leucomaines; (2) that these induce fatigue and sleep; (3) that on waking, if the body is rested, these substances have disappeared.

To complete the demonstration of these statements much careful experimentation is necessary; but the facts as far as they go make it probable that the chemical theory of sleep will gain in strength as our knowledge advances.

ETHNOLOGICAL NOTES.

THE HAWAIIAN ISLANDS.—Dr. E. Arning's researches have been very successful. He was sent there by the curators of the Humboldt Fund at Berlin, in order to study leprosy, which has recently become the plague of the natives of this group. He stayed there for two and a half years, and during this time carefully collected relics of the ancient Hawaiian culture, and succeeded in bringing to light many points of interest, thus proving that European influence, which has swamped the islands since 1820, has not totally destroyed the remembrance of olden times. When the missionaries established their schools in Hawaii, the natives rapidly adopted European customs, burnt their temples and idols, and cast the stone images of their deities into the sea. The destruction was so complete that no traces seemed to remain. Arning, in studying the disease mentioned, had ample opportunities to come into contact with the natives in the remote villages of the islands, and here he found still many relics, and received information about the ancient arts and customs. His notes on the fishery of the Hawaiians are of interest. They were skilful divers, and used to frighten the fishes out of the caves and hollows of the rocky ground with sticks, and then catch them in nets. When fishing in the canoe, they used a sacred piece of heavy wood, called *melomelo*, which was kept in the sacred part of the hut, and was placed, with many ceremonies, in the canoe. It was attached to the net in order to attract the fish by its magic spells. A variety of hooks were used for different kinds of fish and according to the time of day, irised shells being applied at noon and in a bright sun, while white ones served early in the morning and late in the evening. Arning describes their games, the wooden sledges on which they used to glide down the steep slopes of the mountains; the remarkable boards of koa-wood, shaped like an ironing board, standing on which they rode through the surf; the *moa*, a spindle-shaped piece of heavy wood, the use of which was allowed to the chiefs alone, who let it glide down the slope of a hill, at the foot of which it had to pass between two poles; and the famous game of *maika*, which is similar to the Italian 'boccia.' At the present time, when a powerful reaction against the missionaries is spreading all over the islands, the old *hula* dance has been revived, and the ancient dancing, ornaments, and musical instruments are used again. Arning describes a foot ornament made of 960 canine teeth of dogs, the work of several generations,—for dogs were slaughtered only at high festivals,—their drums, flutes, and xylophones. Arning's observations and collections form one of the most important recent contributions to Polynesian ethnology, and are the more valuable as they were made in a country which seemed to have lost all its originality by its rapid commercial development.

THE BOTOCUDOS.—Dr. P. Ehrenreich has published the results of his study of the Botocudos of the Rio Doce in the *Zeitschrift für Ethnologie*. He discusses the observations of former travellers, and compares them with his own experience, thus giving the best sketch of this interesting nation which can be obtained at the present time. Dr. Ehrenreich has collected a considerable amount of anthropological, ethnological, and linguistic material. He gives a number of craniological and anthropometrical measurements, sketches the life of the tribes, who live in a remarkably low state of civilization, and gives a vocabulary—which he has compared with the older ones of Martius—and brief grammatical notes. His researches lead him to the conclusion that the Botocudos formerly occupied a more extensive territory than they do at the present time, inhabiting a tract of land which extended from the coast far westward. They are related to the Ges nations, who inhabit the central parts of Brazil, and a member of whom was discovered by Von Steinen on the upper Xingu. It is of importance to know that the Ges and the Botocudos wear labrets and ear ornaments, that their ceramic art and methods of navigation are very primitive, and that they do not use the hammock. Ehrenreich is of the opinion that the Botocudos remained in an earlier stage of development than the Ges nations, who migrated west and came into contact with other peoples, while the former remained isolated. He believes that the remains found in the caves of the province of Minas Geraes belonged to the ancestors of the Botocudos.

ORIGIN OF THE ESKIMO.—In the *American Naturalist* of August, 1887, Mr. Lucien M. Turner criticises Dr. H. Rink's theory. The latter supposes that the Eskimo were originally an inland people, living somewhere in the north-western part of North America, whence they descended to the seacoast along the rivers. In several articles, Dr. Rink tries to prove this theory by comparing the languages and customs of the different tribes. Though convincing proofs cannot be given, it seems very probable that the Eskimo have come from the rivers and lakes in the interior of America. This theory is open to criticism, but Turner's objections fail to convince us, and do not meet Rink's arguments. The latter is right in laying stress upon the fact that the Eskimo are not so exclusively a coast people as is generally supposed. The most difficult problem of the study is the difference of the tribes west and east of the Mackenzie. Rink emphasizes the fact that the former have certain inventions which the latter have not, while other implements are more developed the farther east we come. From this fact he concludes that the Eskimo first reached the sea and came into their present environment west of the Mackenzie, near the mouth of the Alaskan rivers. This theory, though not improbable, ought to be scrutinized by a study of the anthropology of Alaskan and eastern Eskimo tribes. It seems to us that much of the difference may be due to foreign influence. An interesting paper on the anthropology of the Eskimo, more particularly of those of East Greenland, is contained in the *Bulletin de la Société d'Anthropologie* (ix. p. 608). While the population of western Greenland is mixed with Danish elements to such a degree that there is probably nobody of pure Eskimo descent in South Greenland, this tribe has never mixed with Europeans. They are less dolichocephalic and slightly taller than the West Greenlanders and other eastern tribes. Their noses are described as being aquiline, but this also occurs among other tribes. The researches in East Greenland which were carried out by Lieutenant Holm show definitely that the tribes of the east coast never came into contact with the ancient Normans.

BOOK—REVIEWS.

The Treatment and Utilization of Sewage. By W. H. CORFIELD and LOUIS C. PARKER. London, Macmillan. 8°.

THE fact that this work has reached a third edition is evidence of its value and usefulness. Since the second edition was published, sixteen years have elapsed, during which time great progress has been made in the methods of treatment of sewage, so that it has been necessary, in order to bring the book up to date, to incorporate much material which will not be found in the earlier editions. The historical portions have been retained in their entirety, as being not only interesting in themselves, but also, on the one hand, descrip-

tive of a state of things still to be found in many places, and, on the other, important as a record of methods and processes which have been adopted at various times, for methods and processes which have been tried and abandoned as useless are liable to be brought forward again as new at some future time unless such a record is kept. Special attention has been given in this edition to the important investigation of the British Association Sewage Committee, more especially as regards the determination of the percentage of the manurial ingredients of sewage actually utilized by irrigation on land, and recovered in the form of crops, and the accurate method devised by that committee for taking samples of sewage and effluent-water for analysis. The practical inquiry originated by the suggestion of the late Dr. Cobbold that entozoic disease might be spread through the agency of sewage farming, and the quantitative examination, with a view to its manurial value, of the compost resulting from the use of earth-closets, are described in detail. The table of contents is a very extensive one, occupying twenty-two pages, and includes many subjects of great interest and importance of which the title of the book gives no suggestion.

In the opening chapter reference is made to the early systems for the collection and removal of excreta, the midden heaps, the stagnant ditches, and the open cesspools. In some of the English towns, in 1845, the privies were in the cellars, and often overflowed. This condition of things could not but be detrimental to health, and must of necessity favor to an alarming extent the spread of many epidemic diseases. Those who question the relation between filth and disease will do well to read that chapter in Dr. Corfield's book in which he treats of this subject. He succeeds in demonstrating that the opinion that the pollution of drinking-water by excreta, and of the air by emanations from cesspools and so forth, on the one hand, and on the other the amount of general sickness, and, in many cases, of special epidemics, stand in the relation of cause and effect, is a true one. Instances are given of fever, cholera, and other forms of disease, breaking out in English towns, which are directly traceable to the filth which had been allowed to accumulate.

In the reports of the Health of Towns Commissioners it is continually pointed out that sickness is the chief cause of the non-payment of rent. One witness says: "Three out of five of the losses of rent that I now have are losses from the sickness of the tenants, who are working men. Rent is the best got from healthy houses." Another says: "Sickness at all forms an excuse for the poorer part not paying their rent, and a reasonable excuse," so that filth causes sickness, sickness inability to work, inability to work poverty and non-payment of rent, to say nothing of starvation. We not infrequently hear in this country, the statement that the State has no right to interfere, that a man's house is his castle, and that he can do what he likes within it. It is this sentiment which for so many years prevented legislation for the protection of tenants in our large and dilapidated tenement-houses, a sentiment which is, we are glad to say, being done away with, more, however, we fear, because the laboring men are beginning to realize and exercise their power than because of any general awakening of landlords to the duty which they owe to their fellow-men. Writing on this subject in 1844, with reference to the then state of Liverpool, Mr. Howe said: "The man who, in a crowded street, is living in filth and breathing a putrid atmosphere, or who makes that street a receptacle for the offal which he casts from his dwelling, becomes the instrument of danger to his neighbors by spreading infection, and he not only hazards his own life, but endangers that of others. The man who erects a flimsy edifice in a crowded thoroughfare, which by its falling may destroy life, should be prevented doing so; and he who constructs a house to let for profit and pays no attention to those matters which are essential to comfort, but, on the contrary, so constructs it as to engender fever and endanger the lives of his tenants,—all these are cases where, with propriety and in justice the legislature ought to interfere, and to insist upon such a mode of construction as will not endanger human life." The earth and ash closets are fully described and their advantages and disadvantages discussed. In speaking of this system, Dr. Corfield says that there can be no doubt that a well-managed dry-earth conservancy system, or midden and ash-pit system, is better than no system at all, but it by no means follows that they are free from danger. They both go upon a wrong principle: we do not want conservancy at all; our first object must be

to get rid of refuse-matters, and not to see how long we can keep them about our houses in a *presumed* harmless condition. The Rivers Pollution Commissioners in their first report, 1868, have no hesitation in pronouncing the dry-earth system, however suitable for institutions, villages, and camps, where personal or official regulations can be enforced, entirely unfitted to the circumstances of large towns.

The subject of sewerage is very fully treated. In the consideration of the separate system, that at Memphis, Tenn., and Pullman, Ill., are mentioned and described. The best method of sewer-ventilation is still undetermined in this country, and the sanitary journals are at the present time discussing the subject with a good deal of earnestness. On this point Dr. Corfield says that the very common plan of ventilating sewers by means of untrapped rain-water pipes from the roofs of houses is extremely dangerous. These pipes are often very loosely jointed, and the air rising from the sewer will escape through every such joint, possibly into bedrooms; and in many cases the open head of the pipe is just beneath a dormer or attic window. During heavy rain the rush of water down these pipes will force the air of the drain into the interior of the house through trapped or untrapped openings. He also condemns the practice of ventilating the sewers by means of the soil-pipes of houses, as there is constant risk of the escape of sewer-air through defective joints into the interior of the house. The house-drain should pass through a disconnecting chamber with an air inlet, and be trapped before entering the sewer. Connecting the sewers with furnace chimneys is also condemned. The Shone, Liernur, and Berlier systems of sewerage are described in a concise but thoroughly intelligible manner.

Among other interesting topics discussed, and which we are compelled to pass over for want of space, are the sanitary aspects of the water-carriage system, the value of sewage, the injury which it works to rivers, the pollution of drinking-water, the discharge of sewage into tidal waters, the straining and precipitation of town-sewage, filtration, irrigation, and the treatment and utilization of manufacturing-refuse. In speaking of the influence of sewage-farming on the public health, the author states, that, as far as nuisance is concerned, there is no doubt that if irrigation farms are badly managed they may be made a nuisance to the neighborhoods. Ordinary sewage is only in a very slight degree offensive when fresh. What is really the most offensive part of sewage farms is the black slimy mud which collects along the sides of the carriers when the sewage is not filtered before being sent to the fields. It is advisable that sewage should be filtered and strained in the manner practised at several places. There is no reason to spread a layer of comparatively worthless and necessarily offensive filth over the surface of the soil. There is good reason to expect that the utilization of the sewage of towns on the land near them, while preventing the pollution of drinking-water, and the spread thereby of cholera and typhoid fever, will at the same time maintain the purity of the atmosphere around and about the towns, and that the result will be, especially when combined with that produced by the increased demand for labor and the more plentiful supply of food, a diminution of the general death-rate.

The late Dr. Cobbold had great fear that entozoic diseases would be spread by means of sewage irrigation. Although this possibility has been borne in mind ever since Dr. Cobbold drew attention to it in 1865, there are no facts reported which connect entozoic diseases with sewage irrigation. Dr. Corfield summarizes his views on the question by saying that it has not yet been shown that sewage irrigation has ever increased the amount of entozoic disease in men or cattle. Still less that it is likely to do so to a greater extent than any other method of utilizing human excrement; and were this shown to be the case, the danger would be to a great extent obviated by some preliminary treatment, with a view to the separation of the suspended matters.

The Treatment of Sewage. By Dr. C. M. TIDY. New York, Van Nostrand. 24°.

THIS little book, which is one of Van Nostrand's 'Science Series,' contains in a very concise form a great deal of valuable information on the subject of which it treats. It goes over necessarily much of the same ground as Corfield's 'Treatment and Utilization

of Sewage,' a review of which we have already given, but in a much more condensed form.

Dr. Tidy, in marked contrast with Dr. Corfield, thinks that there is danger that entozoic diseases may be communicated to both man and beast by means of the products of sewage farms. He says that the fact has always been recognized that entozoic diseases have an external origin; i.e., that the ova or parasites come from without, and are not generated within, the human body. Millions of ova are voided with every segment discharged by the person afflicted with tapeworm, each ovum being capable of producing a measles in the flesh of an animal, and each measles a tapeworm in the body of the man. He has seen watercresses and celery grown on sewage ground, having a quantity of dried sewage matter deposited on the stems, and he has, with more than a cook's patience, tried to wash this matter off, but the tenacity with which it sticks upon the surface of the vegetable when once dry is perfectly astounding. It should be remembered in this connection that these vegetables are eaten in an uncooked state. The grass covered with sewage, eaten as it is with rapacity by the cattle, infects their bodies with the larval parasite. Thus the meat is measly, and measly meat, except for efficient cooking, means tapeworm to the human subject. Perhaps a similar story might be told of trichina, with its ten times greater danger. The farm, therefore, that receives sewage must be more liable to produce measly meat than the farm that does not receive it.

In opposition to these views of Dr. Tidy we have the opinion of Dr. Corfield, already referred to, and also that of the British Association Committee. This committee made experiments to determine this very question of the distribution of entozoic disease by means of sewage irrigation. Dr. Cobbold, at the request of this committee, examined the carcass of an ox which had been fed for two years on sewage-grown grass, and reported the perfect freedom of that animal from internal parasites of any kind, but explained this freedom in a manner which to his mind did not affect the main question. The committee did not accept this explanation, but in their report say that it appears as far as this one case goes (and it is certainly as conclusive as a single case could possibly be), there is no evidence that entozoal forms of life are to be found upon the farm at all, in any stage of their existence, or in the flesh of an animal fed exclusively for twenty-two months on sewage produce grown on the farm. This report was made in 1871, but we have Dr. Corfield's statement that since the date of that report no facts have been recorded connecting entozoic disease with sewage irrigation.

It would be interesting to know whether Dr. Tidy or others have any evidence to the contrary. It would seem as though the system had certainly been in practical operation long enough to have settled this question.

It is a matter of regret that the publisher of Dr. Tidy's book has not given the reader a table of contents or an index. In order to ascertain what it contains it must be read through from title-page to colophon, and as a book of reference its value is greatly diminished from this omission.

NOTES AND NEWS.

A SANITARY Convention was held at Traverse City, Mich., Aug. 24 and 25, under the auspices of the State Board of Health. The objects of the convention were the presentation of facts, the comparison of views, and the discussion of methods relating to the prevention of sickness and deaths, and the improvement of the conditions of living. It was not a doctors' convention, but for the people generally. Among the many subjects which were presented and discussed were the following: disposal of waste in Traverse City by sewerage and otherwise, the present and future water-supply of Traverse City, the best methods of warming and ventilation, the work of the village health-officer, the money value of sanitary work, the prevention of contagious diseases, school hygiene, foods and their adulterations, the drink problem, and the prevention of insanity.

— In the letter on 'Chrome considered as a Poison,' by Charles Harrington, in last week's *Science*, centimetre (p. 105, col. 2, 4th line) and centigram (p. 106, col. 2, 21st line) should read 'gram.'

LETTERS TO THE EDITOR.

* * The attention of scientific men is called to the advantages of the correspondence columns of *SCIENCE* for placing promptly on record brief preliminary notices of their investigations. Twenty copies of the number containing his communication will be furnished free to any correspondent on request.

The editor will be glad to publish any queries consonant with the character of the journal.

Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.

Wind Pressure and Velocity.

THE importance of an accurate determination of the relation between the pressure and velocity of the wind will be readily recognized. This relation is especially needed by the architect and bridge-builder, since most instrumental determinations are of the wind's velocity. The problem is much more intricate than is ordinarily supposed, and the diverse results obtained by experimenters of great ability show how the determination of the movements and behavior of gaseous media are hedged about with difficulty, and, as already pointed out in *Science* for July 8, the absolute necessity of building up the science of meteorology on a firm foundation of fact rather than theory.

There have been two methods of experimentation: the earliest, with plates rotated upon an arm seldom exceeding 10 feet in length, and, later, by the exposure of plates to direct air-motion. Borda, in 1763, with plates ranging from 16 to 85 square inches area, obtained the following relation,

$$p = (.0031 + .00035 c) S v^2,$$

in which p = pressure in pounds on the plate, c = contour of plate in feet, S = surface in square feet, and v = velocity in miles per hour (this notation will be maintained throughout).

In 1874, Hagen tried most careful experiments with an arm of 8 feet. The velocity ranged from 1 to 3 miles per hour, but the room was so small that at the latter velocity the air was set in feeble rotation. The plates ranged in size from 4 to 40 square inches. He found, as did Borda, that the pressure per square foot increased with the size of the plate. The following is the relation established by him,

$$p = (.002894 + .0001403 c) S v^2.$$

This formula for this relation has repeatedly appeared in print, and each time it has been changed. This is believed to be correct.

Singular as it may seem, these experiments have been almost the only ones quoted in discussions of this question, and yet it is easy to see that they are utterly useless for determining the pressure of a 50-mile wind on the side of a building.

In November, 1886, a few experiments in Washington with an arm of 4 feet, and plates from 16 to 576 square inches area, gave the relation,

$$p = (.0032 + .00034 c) S v^2.$$

The agreement with Borda's results is very interesting.

Afterward, with the same style of apparatus and an arm of 16 feet, the relation found was

$$p = .0034 S v^2.$$

The velocity of the larger plates was only 4 miles per hour, so that this formula does not help us for greater velocities. It was certainly established that there was no difference in pressure per square foot depending on the size of the plate. Turning to experiments of the second class, we find that Thibault obtains, with plates from 1 to 1.5 square feet area exposed to the wind, the relation,

$$p = .00475 S v^2.$$

In France, with plates exposed on a locomotive running 44 miles per hour, the relation established was

$$p = .00535 S v^2.$$

In this case, probably, a slight allowance must be made for the wind with the train.

In the 'Encyclopædia Britannica,' article 'Hydromechanics,' the mean of all the better determinations is

$$p = .00496 S v^2.$$

We may conclude, 1st, that experiments with whirling arms of less than 16 feet are very untrustworthy; 2d, that we need determinations with rapid, straight-line motion, best obtained, perhaps, by pushing two or three platform-cars loaded with iron in front of a locomotive, exposing the plates on the front car; 3d, the relation

$p = .005 S v^2$ is the most satisfactory yet determined, and does not differ by more than four or five per cent from the truth.

While there has been this great difficulty in determining the above relation, there has been just as much, if not more, in connection with the relation between the velocity of the wind and that of the cups of Robinson's anemometer. Some confusion has arisen from the fact that the standard anemometer in England has 9-inch cups and 24-inch arms, while in our country we have 4-inch cups and 7-inch arms.

It has been determined, by careful experiment in England, that, if the large type of anemometer has a factor of 2.5, then the smaller should certainly have 3.00. Dr. Robinson, after a long research with a whirling machine, decided that the factor (of the smaller instrument probably) should be about 2.5. After trying a few experiments in the open air, however, he changed his view, and decided that the factor should be 3.00. In Washington, with an arm of 16 feet, and a velocity of 12 miles per hour, the factor was found to be 3.00.

Quite recently the Chief Signal Officer, through the kindness of the officials, as a preliminary to carrying on experiments on platform-cars, as suggested above, has had an anemometer placed upon a locomotive of the Baltimore & Ohio Railroad running from this city to Baltimore. Only one round trip has been tried thus far: in the outward trip the velocity of the train was about 20, and returning it was about 46 miles per hour. Allowing for the actual wind, we find the anemometer indication 46 miles going, and 47 returning. The distance was 40 miles, and we may consider that the excess of about 6 miles was due to the heaping up and flowing over of the air in front of the locomotive. All things considered, it seems probable that the factor 3.00 now used in our anemometers of 4-inch cups and 7-inch arms is entirely correct: certainly no change in the present factor can be thought of for an instant. A complete discussion of this question has already been prepared by me, and will appear in October. The other side of this question has been recently presented by Professor Ferrel in the *August American Meteorological Journal*. H. ALLEN HAZEN.

Washington, Aug. 22.

The Formation and Dissipation of Sea-Water Ice.

MR. W. A. ASHE'S opinion on the freezing-point of sea-water, and the conclusions he draws from his experiments, cannot be accepted. The arrangement of the experiment described in No. 228 of *Science* seems to be insufficient. A hole was cut through ice 87 centimetres (2.85 feet) thick. The water within was thoroughly agitated by stirring from below, and during the actual observation slightly agitated. The thermometer was held nearly horizontally, the bulb slightly lower than the rest of the instrument, just below the surface of the water. When the ice-film began to form, the reading of the thermometer was $-2^{\circ}.9$ C. ($26^{\circ}.7$ F.), the temperature of the air being $-24^{\circ}.8$ C. ($-12^{\circ}.6$ F.). The greatness of the difference between the freezing-point of the sea-water and the temperature of the air detracts from the value of these observations. The ice is forming so rapidly that brine is included among the crystals: it is even probable that cryohydrates are formed at the surface. On the other hand, the freezing-point of sea-water was not only found by melting sea-water ice, as Ashe assumes, but also by freezing sea-water, and was always found to be between $-1^{\circ}.6$ C. and $-1^{\circ}.8$ C. ($29^{\circ}.1$ and $28^{\circ}.8$ F.), according to the concentration of the solution. Mr. Ashe's second remark on this subject in No. 232 of *Science* does not agree with Buchanan's interesting researches on the melting of fresh-water ice in solutions of salts. He has shown by an excellent series of experiments (*Nature*, April 28 and May 5, 1887), that, when sea-water is frozen to the extent of fifteen per cent of its mass, and the crystals so formed are allowed to melt in the liquid in which they have been produced, they melt exactly as they have been formed. If snow or pure ice be immersed in the brine formed by partially freezing sea-water, it melts at the same temperature as the ice which had been formed by freezing the sea-water, so long as the chemical composition is the same in each case.

In a third letter to *Science* (No. 237), Mr. Ashe makes some remarks on the formation and character of Arctic ice. He says, that, as the density of sea-water increases till the freezing-point is reached, ice is not formed at the surface, but at a certain depth. In fact, the

cooling goes on from the surface, and is far more rapid than the motion of the water to deeper layers: therefore ice is formed before the cooled masses sink to the depth and are replaced by warmer ones.

I can confirm Mr. Ashe's observation that the new ice frequently forms first on projecting points. It seems to me that particles of ice which have formed on the shore are driven by wind and waves into the sea, and act as so many centres of congelation. As these particles and small cakes are drifting before the wind, long, narrow streamers are formed, each new opening being rapidly filled by new ice. These narrow strips do not consolidate, but are driven to a lee coast, or their motion is stopped in some other way. Then they are pressed together, the small cakes are broken and overflowed by ripples, the water freezes on top of them, and within a short time an extensive field is formed consisting of numerous small cakes pressed and piled upon one another, and cemented by the water that has overflowed them. Besides this kind of ice, which is formed while the wind is blowing, smooth floes are formed in small bays during calm weather. The latter, however, form the smaller portion of Arctic ice. This process accounts for the great quantity of brine contained in the new ice. The deeper layers contain far less salt than the surface ice, as the mechanical admixture of water takes place on the surface only. The snow, on falling on this kind of ice, forms a slush, as it melts in the brine oozing out of the ice. This mixture freezes, and thus the thickness of the ice increases on the upper and lower sides simultaneously. The different origins of the layers of ice accounts for their different character.

During the winter the ice undergoes remarkable changes. The brine contained in ice of different depths is the more concentrated the lower the temperature of the layer. It is probable that the cavities between the ice-crystals are sufficient to effect a gradual decrease in the concentration of the upper parts, which then begin to freeze, and thus become drier.

When in spring the ice begins to melt, it loses its salt rapidly, and I observed in latitude 70° north, on the west coast of Baffin Bay, that it had become entirely fresh about the end of June, the brine being removed through the capillary cavities.

A remarkable effect of mechanical action upon the dissipation of the sea-water ice may be observed at places where rapid tides are running. When the temperature of the air rises to about —20° C. (—4° F.), the ice becomes saturated with water, and is being worn off at its lower side. It seems that the lower surface of the ice is rough, consisting of ice-needles with isolated points. These are broken off by the violent motion of the water, and thus by the friction of the broken pieces the volume of the ice-sheet is continually being diminished at its lower side. In winter the same process must be going on, though not to so great an extent. Some places of this kind are open throughout the winter. Their extent is changing according to the strength of the tides; and during the spring-tides ice of about fifteen or twenty centimetres thickness, which was formed during the preceding neap-tide, is broken up and pressed under the neighboring floe: consequently the thickness of this ice would continually increase if it was not worn off at its lower surface. In fact, the ice in such places is very treacherous and thin: therefore it seems, that, according to the strength of the current, a certain low temperature is required to resist its destructive influence. Below the layer which has this temperature the ice is saturated with water and being worn off.

Mr. Ashe touches upon another subject the explanation of which seems to me insufficient. On steep coasts in the Arctic regions an ice-wall is found attached to the rocks, and reaching from high-water to low-water mark, gradually decreasing in thickness, and having a vertical side. When the tide ebbs, the wet rocks are exposed to the cold air, and of course are covered with a thin sheet of ice. This process begins before the sea is frozen over, and, as the water is still agitated by winds, the thickness of the layer formed during a single tide is considerable. This process is going on throughout the winter, and thus this ice-foot continues to increase in width. At spring-tide its level is overflowed by water, which adds to its height. It is characteristic of this ice-foot that the unbroken land-floe extends to its foot, a single crack separating the two masses. Cliffs which are washed by water throughout the winter have frequently no ice-foot at all. This shows that the floe favors its formation.

The ice on shores with a gradual slope is quite different. Here it is not attached to the bottom, as Mr. Ashe's description would imply, but originates in the following way. The ice which is formed on the surface at high water strands during the ebb-tide, and its volume is increased by the freezing of the water left on the beach. When the tide comes in again, the greater part of this ice begins to float, increases in thickness, and strands again on the rocks. By the repeated breaking of these masses on the rocks, the water is frequently exposed to the air, freezes, and thus the thickness of the ice is far more rapidly increasing than that of the unbroken floe. It is continually growing on all sides, new material being also added on the sides, and thus a heavy pressure results which affects the neighboring parts of the floe, which is frequently pressed under the level of the sea, is overflowed, and thus increases in thickness. If the slope is sufficiently gentle, this part of the floe strikes also the rocks at low water, and is added to the growing belt of grounded broken ice that surrounds the coast. Similar forms of ice are found on mud-beaches, and I believe that such is the origin of the unusually heavy mud-colored masses of Fox Basin, which is known to be extremely shallow.

The formation of the ice-foot which was described above is similar in origin and appearance to the frozen freshets which in winter form ice-walls that are firmly attached to the steep cliffs. This proves that Mr. Ashe's explanation of this fact is not correct, and that fresh water as well as sea-water may freeze firmly to the ground.

DR. FRANZ BOAS.

New York, Aug. 19.

The Geologists' Congress.

PERMIT me to say a word concerning the generally fair and full report, in *Science*, of the proceedings in Section E of the American Association. By a typographical error, a clause in the conclusion of the digest of the reporter on the Archæan is made to read "American geologists will acquiesce in the recommendations of the committee," etc. 'Congress,' not 'committee,' was the word used. Again, "the recommendation that all pre-Cambrian rocks should be called Archæan savors too much of pre-judgment, especially in view of the recent studies of Irving and Walcott."

These studies, which have resulted in the theory called by Professor Walcott, in his letter to the reporter, 'Prof. R. D. Irving's view,' were very carefully considered in the body of the report. The sentence above gives no idea of the entire recommendation; of how far it differs from the view of Irving, Chamberlin, Walcott, and others; nor of why it seems to best reconcile the conflicting views expressed by American geologists. 1st. The term 'Archæan' as originated by Dana received the unanimous suffrages of the Berlin Congress, and, so far as the reporter could ascertain, has the indorsement of a very large majority of American geologists as a general term to cover all pre-Cambrian rocks. Irving would separate these rocks into two divisions of equal rank: to the lower (Laurentian) 'Archæan' should be applied, the upper (separated by a physical break from the first, and containing an unspecified number of smaller breaks or unconformities) he would erect into a new group of equal rank with Archæan in its new sense, and would call it, after Chamberlin, 'Agnotozoic.' He recognizes "great unconformity between the Cambrian and the Agnotozoic, besides which there are minor, though still quite extensive, unconformities between the members of the Agnotozoic itself." It is evident, from the tenor of all the views expressed on this proposed new division, that its exact rank is not certainly understood. All who recognize it believe that it has at least as high a rank as 'Paleozoic,' 'Mesozoic,' etc., but none can yet affirm that it may not consist of several such groups, divided by one or more of these 'extensive unconformities.' In view of this fact, and also of the circumstance that to a vast majority of geologists to-day the Archæan includes all pre-Cambrian rocks, it was thought that the recommendation of the reporter avoided, to the greatest degree, any pre-judgment of this question. It is as follows:—

"The division first in order of time shall have a rank of the first order, and shall be called 'Archæan.' (a) It shall comprehend all the rocks of origin anterior to the Cambrian. (b) The lowest subdivision of the Archæan shall be called the 'Laurentian.' (c) A division between the Laurentian and the Cambrian, provisionally

including the Huronian, Grand Cañon, Llano, Montalban, and Taconian (of Hunt), Animikie, and other divisions, shall be accorded a name different from any of these (such as 'Eozoic' or 'Proterozoic'), and allowing the greatest amount of liberty in the future, when it shall be determined whether this division shall be entitled to rank as one or several of the first order having numerous subdivisions as above mentioned, or with, or including, any of them of the second order like the class Laurentian. No attempt shall be made at this time to pre-judge this question, and these names and this classification shall be regarded simply as the best that can be accomplished at the present time."

This plan would seem to be possible of acceptance both by those who, like Dr. Hunt, recognize many divisions, and by those who recognize but two. The only sacrifice of individual opinion required would be as to the rank of such divisions, which the reporter doubted whether any geologist wished unreservedly to affirm. When the exact rank of the proposed new division is generally accepted, if it be a group, the now all but universally accepted word 'Archæan' can be dropped or otherwise assigned.

As to the resolution approving the action of the committee, if it only received two or three affirmative votes, it is equally true that it received no negatives.

Philadelphia, Aug. 25.

PERSIFOR FRAZER.

The Pronunciation of 'Arkansas.'

I HAVE read with much interest Mr. Robert T. Hill's vigorous protest, in the last number of *Science*, against the mispronunciation of this word. Nevertheless, it seems to me that Mr. Hill, whose personal acquaintance with New England is comparatively recent, has been unintentionally not quite fair to "intelligent New England circles," in making them responsible for the "later and improper pronunciation." I am a New Englander by education, myself, and was taught, before I went to school, to pronounce the word properly. In school, however, our teachers insisted on the 'revised version.'

I am pretty well convinced that the mispronunciation was the invention of a class of school-teachers, unfortunately too common in New England, whose training for teaching the 'English branches' is so specialized as to carefully exclude every thing relating to foreign languages (including even the English of *Old England*). Not a few other examples might be quoted of similar 'school-ma'am' pronunciations. 'Glou-ces-ter' and 'Wor-ces-ter' are beginning to replace the proper sounds among the younger generation of 'common-school' scholars in New England, at least, and 'Norwich' and 'Harwich' are well established. It seems to me that we really do need more such protests as Mr. Hill's, before the rage for anglicizing does away with the historical pronunciation of more of our geographical names.

In regard to the word 'Cheyenne,' I suspect that Mr. Hill has laid the blame on the wrong shoulders. How the western plainsmen (who, one would suppose, would have inherited the correct pronunciation, or something like it, from the old *coureurs de bois*), came to call the 'Dog Soldier' band of Indians 'shy-ens,' instead of 'chiens' I cannot say. I do know, however, that this was the established plains pronunciation. We can scarcely blame the New England lexicographers, — or whoever first wrote the word, — therefore, for failing to recognize the French word under the universal Western pronunciation.

JOHN MURDOCH.

Smithsonian Institution, Aug. 27.

Eskimo and the Indian.

IN an article on the Eskimo of East Greenland in the current number of the *American Naturalist* (p. 749), it is stated that the eminent savant Dr. Rink has recently advanced the idea that the 'kayak' of the hyperborean American aborigines is but a development from the birch-bark canoe of the neighboring Indian tribes. In glancing through Petitot's 'Tchiglit (Mackenzie River) Dictionary,' I found what seems to be a confirmation of this theory. In the Tchiglit dialect the word for boat is *krayark*, and the bark of the birch used for canoes (*écorce du bouleau à pirogues*) is called *kreyrork*. A comparative study of the Eskimo and the neighboring Indian dialects must certainly result in adding considerably to our stock of knowledge regarding the interesting Innuits. A few ex-

amples of Eskimo loan-words may be given here. In the dialect of the Eskimo of Churchill River the word for 'dead' is *nipa*, which agrees with *nipiw* (Cree), *nip* (Chippeway), etc., being entirely foreign to the stem *tok*, which pervades the Eskimo dialects from Cape Farewell to the Anadyr. One of the Tchiglit words for 'rain' is *nipaluk*, evidently related to the Cree *nipi* (water). In Algonkin we find this series: *nipa* (moon), *nip* (die), *nipi* (water); and it is worthy of attention that this peculiar concatenation is repeated with the Eskimo of whom I am speaking, viz., *nipa* (dead, in Churchill River dialect), *nipaluk* (rain, i.e., water, in Tchiglit), *nipartvark* (a Tchiglit term for moon). In the far west we find the words *madsschak* (sun, in Kadiac), *matschak* (sun, in Anadyr Tchuktchi), *madje* (sun, on Kotzebue Sound), *madzak* (star, in Kadiac), which bear a suspicious resemblance to *maitsaca* (moon, in Tarahumara), *matzake* (moon, in Cora), *mecha* (in Cahita), and *metztli* (in Aztec), and would seem to indicate the great northward extension of Aztec influence along the coast of the Pacific. Thorough research will no doubt reveal much that is interesting and valuable in this regard.

A. F. CHAMBERLAIN.

Toronto, Aug. 25.

Sea-Water Ice.

IN *Science* for August 19, under the heading 'The Formation and Dissipation of Sea-Water Ice,' Mr. Ashe, in speaking of the formation of ice along the shore, and the accumulation of films of ice upon that which is submerged, makes the following statement: "Over this, at the surface of the water another film is formed, which, on reaching the level of the submerged ice, is frozen to, and remains with it in this position. This operation is repeated till the result is that a perpendicular wall of ice forms, whose outer limit is the low-water mark, terminated by a horizontal surface shorewards, at the limit of high-water mark."

If it is meant to convey the impression, as it would naturally be supposed, that this is so in all parts of the world, I must flatly contradict the statement for Cape Prince of Wales, Hudson Strait, where I was stationed, at the head of a sandy bay, during the winter of 1885-86, with the object of watching the formation and dissipation of sea-water ice. Here the distance between high and low tide mark was about three hundred yards, and, although some ice did adhere to the sand, it always came to the surface in irregular pieces shortly after the tide had risen above it. These pieces were often piled one upon another by the force of the wind, accumulated, and as the winter advanced they rose to the surface in larger masses, until the ice in the bay had reached a variable thickness under three feet, when the whole mass floated at each rising of the tide as one piece, only cracking in a few places, and, with the exception of its rough surface, no change of level could be noticed between it and the ice always floating beyond low-tide mark.

A higher tide than usual always forced its way through, within a few feet of high-tide mark, the ice cracking with a loud report here and there along the shore, the water again returning through these cracks when the tide began to fall.

Late in January a hole was cut through the ice between high and low tide mark, when the sand was found to be perfectly soft, in which living shell-fish were found.

F. F. PAYNE.

Toronto, Aug. 23.

Answers.

14. AN EXPULSION OF SPARROWS. — It was probably a flock of 'white-bellied swallows' that W. A. G. saw circling about his house on Staten Island, but they in no way caused the disappearance of the sparrows as intimated. These swallows are here now in great numbers, perching on telegraph wires and along the seashore on beach, plum, and bayberry bushes, and on hazy mornings many may be seen flying south along our shore line. As to the English sparrows, a few still remain about the houses, but this is their season of flocking, and in some fields, especially where grain has been raised, they abound. I once knew of a double row of elms where these birds congregated afternoons in late summer, and chattered in great convention until the sun went down. They were gathered from a large circle of country, and I think that W. A. G. will find a similar meeting place, where the missing sparrows will be assembled.

WM. T. DAVIS.

Tompkinsville, S.I., Aug. 29.